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POLIOMYELITIS (INFANTILE PARALYSIS)

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THE essential nature of infantile paralysis has been determined within the twenty-year period during which the disease, escaping from its endemic home in Norway and Sweden, has made its epidemic progress over the world. Only the tropics, and even they not wholly, have been spared its ravages.

It is probably just because infantile paralysis had never before prevailed in a world-wide epidemic that we are witnessing the periodical outbreaks which are so tragic in their consequences. On the whole, the outbreaks have been larger and more severe in North America than elsewhere and for reasons, as will appear, which are bound up with the nature of the disease.

Infantile paralysis is a disease long known to physicians, although its infectious and communicable nature was established only about forty years ago during an epidemic in Stockholm. Hence it is known to be a disease of microbic origin. The microbe or microorganism which induces infantile paralysis is so minute that it is not certain that it has ever been seen under the microscope. Because of its minuteness, it can pass through filters of earthenware which hold back, and prevent from passing, ordinary microorganisms, such as the bacteria.

The microbe of infantile paralysis differs in another way from the usual bacteria. While bacteria are easily made to grow outside the body, the microbe can be made to multiply in this way only under special conditions. These conditions are provided by growing and multiplying tissue cells taken from warm-blooded animals and propagated in tissue cultures. When the invisible microbe of infantile paralysis is cultivated with tissue cells, both increase together, the microbe probably within the cells.

There are many other kinds of invisible microbes

which produce diseases of plants and the lower and higher animals. The microbe of infantile paralysis belongs to this class of ultraminute living bodies. All these ultramicroscopic disease-producing microbes share the peculiarity that in order to be made to multiply outside the animal or plant which they attack as parasites, and in which they cause disease, they must be cultivated with living and growing tissue cells.

Certain pathologists have expressed skepticism about the microbe of infantile paralysis because of its invisibility and the difficulty of making it increase outside the body. This would be equivalent, let us say, to denying the existence of atoms because they also are beyond visibility and can not be made to increase at all by any means known to us at present. Now, physicists and chemists are not doubtful about the atom because of certain known properties concerning it. These properties enable physicists and chemists to experiment with the atom and to learn a vast number of things concerning its nature through the experiments. They are in no essential way hindered from acquiring this knowledge because of the minute size of the atom. In a very similar way pathologists have dealt and are dealing successfully with the invisible microbes of disease and prominently among them the microbe of infantile paralysis. A vast amount of new knowledge of infantile paralysis has been obtained by experiment since the microbe was discovered in 1910 simultaneously at the Rockefeller Institute in New York and the Pasteur Institute in Paris.

The control of any communicable disease is greatly promoted by the discovery of the mode of infection or the way in which the microbe enters and leaves the affected body. The epidemic in Stockholm and its environs in 1905 led to the recognition that infantile paralysis was passed from person to person. But just how this passage took place could be conjectured but not definitely determined at the time.

With the discovery by Dr. Landsteiner in 1909 of the communicability of infantile paralysis to monkeys the way was suddenly opened to the detection of this essential fact and many other important facts regarding the disease which have since been brought to light. The first step forward consisted in the discovery at the Rockefeller Institute that the microbe of infantile paralysis escapes from the body in the secretions of the nose and throat. It is not as widely appreciated as it should be that a way for the disease-producing microbe to escape alive and undamaged from the body is just as essential for the spread of the disease as a way of effectively entering a healthy individual in whom disease is to be produced. Hence the detection of the manner and place of exit of the microbe may afford the clue as to its means of entrance. And this is actually what happened in the instance of poliomyelitis. Finding that the microbe escapes by way of the nose and throat led almost immediately to the discovery that it was able also to enter the healthy monkey and induce paralysis through these organs. This knowledge led in its turn to the detection of the microbe in the secretions of the nose and mouth of children suffering from infantile paralysis and also in these secretions of persons—parents especially—who have been in intimate contact with the sick children.

At the present time it is the firm belief of most pathologists that the microbe of infantile paralysis is carried from person to person, from the infected to the uninfected, through the secretions of the nose and throat. The public health measures designed to reduce the spread of the disease are based on this belief. All this does not mean that there may not be still other ways of communicating the disease yet to be discovered. It is known, for instance, that the microbe of infantile paralysis can be spread by milk. Two small, isolated outbreaks of the milk-borne disease have been detected in New York State within a few years. These epidemics differ from the usual ones in their restricted extent and almost explosive character. The cases tend all to arise in rapid succession, after which there is complete or nearly complete cessation. A few days or a week or two witness the entire progress of these outbreaks.

One attack of infantile paralysis, no matter how slight, usually affords protection for life. Associated with this state of immunity, and responsible in part, if not wholly, for it, is the presence in the blood of a substance which, when inoculated into monkeys, is capable of preventing the microbe from producing the disease. The serum of the blood, or its fluid portion separated from the corpuscles, is equally effective, and it is capable, not only of rendering the microbe inactive when the two are mixed in advance, but, also, as was discovered at the Rockefeller Institute in 1910, of preventing the onset of paralysis in monkeys when the microbe is first introduced and the serum from recovered human beings or monkeys is injected 24 to 48 hours later.

This observation is the experimental foundation for the convalescent serum treatment of infantile paralysis which, first applied in man by Professor Netter of Paris in 1911, has now come to be widely employed. The extent to which the serum treatment is effective has still to be determined with accuracy. It is now known not to be of value when paralysis has already occurred. On the other hand, it is believed that the earlier the serum is administered after symptoms appear the better the results obtained.

Not only are the protective and curative properties present in the blood of persons who have recovered from obvious attacks of infantile paralysis; they are present also, in some amount, in the blood of many adult persons who have never suffered from the disease. This is not peculiar only to infantile paralysis. Many affections of microbic origin exist in two forms: one in which disease is frankly present; the other in which individuals exposed to the microbe harbor it for a time during which no symptoms of disease occur, but immunity to the microbe is secured. This is true of such common microbic diseases as diphtheria and scarlet fever, which may be cited as examples of a larger class. That the blood serum of normal adults renders the microbe of infantile paralysis inactive was noted by Anderson and Frost, of the U. S. Public Health Service, in 1911. Subsequent studies on a larger scale, carried out by the Harvard Poliomyelitis Commission under Dr. Aycock, have shown that this sort of unperceived immunization is taking place widely to-day in the United States. It is to this protective process, perhaps, that we have to look finally for the eradication of poliomyelitis from this country. It is probable that the reason northern America has suffered disproportionately from the disease in the past 20 years is that the general population, not having been exposed over a term of years as have many of the European populations, lacks the immunity conferred by the unperceived carriage just described of the microbe of infantile paralysis.

We gain a strengthened impression of this kind of protective mechanism at work in the dark by studying the age frequency of the disease and comparing the frequency with the ages at which the blood protection is demonstrable. In childhood, up to the tenth year, there is little protection discernible; this is the period of greatest frequency of the disease. From ten years on the protection rises and the frequency of the disease diminishes. In adults the protection reaches maximum and the cases minimum figures. The proportion of protected adults increases with the opportunity for exposure to the microbe; hence it is higher in urban than in rural communities. The ages of the children who are now suffering most in the New York epidemic conform to this rule. Indeed, with minor exceptions, the victims of the present epidemic have been born since 1916; far more children under rather than over ten years of age have been stricken.

The employment of convalescent serum for the treatment of early cases of infantile paralysis was begun in 1911. In 1916 Zingher, of the Department of Health of New York City, employed the serum of normal adults for treatment. The logic of this procedure is apparent from what has been stated concerning the antimicrobic powers of the blood of many adults. Probably the concentration of the curative substances is greater in convalescent serum, but this need not always be the case. In the absence of supplies of convalescent serum, normal adult serum may be employed for treatment. It is advisable to combine the serum of a number of individuals, rather than to rely on that of a single person, in order to increase the probability of administering an effective dose.

The convalescent serum has been shown experimentally not only to exert curative (therapeutic) properties but to possess preventive powers as well. Definite observations on this point, having as object a possible application to protective serum injection, were made at the Rockefeller Institute in 1928. At this time attention was directed to the employment of convalescent serum for prevention in an emergency. Fortunately, the case frequency of infantile paralysis is not high, so that resort to this measure will not often become necessary. Moreover, so far as convalescent serum is concerned, in an emergency the available supplies will be required for the treatment of declared cases. On the other hand, abundant supplies of normal adult blood are always procurable. In view of the high proportion of adult city dwellers who are immune, the blood serum of parents or other suitable persons can readily be administered to younger children who are exposed to infection. No assurance of absolute protection can, of course, be given, but, by analogy with measles, benefit may be hoped for or even expected. Time and experience alone will make it possible to ascertain the value of this procedure.

The obvious effect of an attack of infantile paralysis is paralysis of the muscles. The microbe does not act on the muscles directly but indirectly through nerve cells presiding over the muscular movements and located in the spinal cord chiefly. These cells are acted on directly by the microbe and injured; sometimes the injury is so severe that the cells are destroyed outright; sometimes the injury is severe enough to interrupt function for a time only. In the one case the muscular paralysis is permanent; in the other restoration of function occurs. The microbe acts also on the cells and tissues about the nerve cells; the disturbance thus produced affects indirectly the function of the nerve cells. When these indirect effects disappear, with recovery from the disease, renewal of function of the nerve cells and muscles takes place. Time is often required for the complete reversal of the general cell and tissue disturbances during which restoration of muscular power slowly returns. There is a great diversity in the extent and location of the injurious effects of the microbe, and there is very great variation in the intensity of the action exerted so that muscular paralysis, when present, may

be slight or extensive, fleeting or permanent. The extent of the paralysis at the beginning of the attack is no accurate measure of its endurance. Recovery from paralysis on a wide scale is not only possible but often takes place.

RECAPITULATION

The microbe of infantile paralysis is known to belong to the class of invisible, filter-passing micro-organisms to which the name of viruses is applied.

This virus has been found in the secretions of the nose and throat of persons ill of infantile paralysis and of well persons in intimate contact with the sick.

When the virus is applied to the nose and throat of monkeys it passes along the connecting nerve fibers to the brain and spinal cord and induces paralysis similar to that occurring in the human disease.

That communication of the disease from person to person is brought about by personal contact and the transfer of the secretions of the nose and throat of the sick to the well has been established by observation of human epidemics and by experiments on monkeys. Whether or not any other common manner of communication of the disease to man exists is not known. Present public health measures of control of infantile paralysis are based on this mode of personal infection.

An attack of infantile paralysis is protective for life, irrespective of the intensity of the attack.

Persons who have had infantile paralysis possess in their blood certain protective or healing substances which can be used effectively to treat persons sick of the disease, and perhaps to prevent the disease in other and exposed children. It is the fluid portion of the blood that is employed in this way under the name of convalescent serum.

Since many normal adults develop immunity to infantile paralysis as a result of exposure to the virus under circumstances not leading to obvious disease, their blood serum also carries, at times, the protective and healing substances. The serum of these adult persons, which is abundantly available, may sometimes be substituted for the serum of convalescents, which is necessarily limited in quantity.

There are strong reasons for believing that a gradual immunization of the population of the United States is taking place as a result of the epidemics of infantile paralysis which have prevailed in different parts of the country since the large Swedish-Norwegian outbreak of 1905.

The virus of infantile paralysis acts upon the nervous system and especially upon the nerve cells of the spinal cord which control muscular movements. The muscles themselves are not directly affected. Since the virus injures the nerve cells and adjacent tissues with varying degrees of intensity, the effects on the muscles range from very slight to severe paralysis. Even when the paralysis is severe, restoration of motion takes place in part or even wholly as the injurious consequences of the disease subside.

Although the name—infantile paralysis—carries the implication of actual loss of motion by muscles, yet many cases of the disease never show paralysis at all. Indeed, there are reasons for believing that the cases of the non-paralytic disease exceed greatly in number those in which actual paralysis occurs.

Infantile paralysis is mainly but not wholly a disease of childhood. Adults are affected but infrequently. Now that we have learned that young children have rarely and older children and adults have often become immunized through unperceived or subclinical effects of exposure, we can better understand the peculiarities of age and place susceptibilities.

GENETICS AND EVOLUTION[1]

By Professor MAURICE CAULLERY

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It seems to me that one of the functions of a congress such as this one, where for the moment many zoologists of all nations and all interests are gathered, is to examine the situation presented by the great problems which change from time to time but which are never completely solved. Such a study is particularly useful in certain phases when our diverse doctrines tend to clash or are not altogether incompatible. Now this is actually the case at the present time with the problem of evolution. Since the peculiar character of my work as a teacher at the University of Paris, devoted as it is to the study of evolution, has led me to constant reflection on the subject, I thought you might permit me to submit some aspects of more timely problems to the consideration of the congress at this reunion of zoologists.

Indeed the nature of the researches most in favor at the present time compel us to face a situation that exists in other congresses (especially the Genetical) where the problem of evolution is brought up indirectly, and where the mass of zoological data is

[1] Address at the opening of the eleventh International Congress of Zoology, Padora, September 4, 1930. Translated from the author's manuscript by J. C. Greenway, Jr.

screened behind results of special methods, a procedure which often leads to paradoxical conclusions. Certain eminent geneticists are even beginning to think that the conclusions of their scientific studies, being based on precise and methodical experimentation, should force us to abandon the idea of evolution whenever their work contradicts it. Others, without going so far, conclude that evolution should only be accepted to the extent that it is in strict conformity with genetical laws, that all conclusions to which zoology and paleontology lead should be deliberately excluded. I do not flatter myself that I have any solution of the difficulty to offer, nor even a new point of view. I should like merely to sail close to the wind, as sailors say, and to confine myself to considerations of a general nature, and perhaps to provoke useful reflection and discussion. I have already had occasion to set forth my views and three years ago, I heard a botanist, Professor R. von Wettstein, express analogous views at the Berlin Genetical Congress. Even this year I noticed that some of these ideas had been presented at a meeting of the combined German Genetical and Paleontological Societies. There is, moreover, a general uneasiness in regard to these ideas, which, it seems to me, may well have an echo here.

The beliefs held when the generation to which I belong did its apprenticeship in science were very different from those of to-day. Then the problem of evolution seemed much nearer solution than it does now. The theory itself was no longer seriously contested, nor did we hesitate to reconstruct the phylogeny both of larger and smaller groups by means of genealogical trees and a general application of the fundamental biogenetic law. Haeckel was the prophet of those days which have now begun to be remote. To-day we are far more cautious in this matter.

In regard to the mechanism of evolution, too, we believed ourselves to be much nearer the truth. It seemed that we had only to choose between the theories of Lamarck and Darwin, or to complete the doctrine of one of these great forerunners with that of the other. Was not Darwin in his later years the first to recognize that he should have made greater concessions to Lamarck's ideas? Environment appeared without question to be the most powerful agent for the transformation of organisms and it moulded them adaptively to the environment under the control of the struggle for existence and natural selection. When one rereads the works of that time, written by the most orthodox of Darwinists (for example, the beautiful books of A. R. Wallace, inspired by his zoological explorations in the Malay Archipelago), one feels on almost every page that these views are in a sense automatic.

August Weismann initiated a change of capital importance about 1885 when he tore down the supposed proofs of the heredity of acquired characters, proofs that were scarcely thought necessary, so obvious did they seem. And we must surely recognize that, in spite of all efforts since that time, the proof of such heredity, even in limited cases, is still to be furnished. The opposition which Weismann remarked between soma and germ-plasm, and from which he deduced the theoretical impossibility of the inheritance of acquired characters, is undoubtedly too radical. And yet we can not deny that his contention has been at least one of the pivots of biological thought in the last decade, and that his theory of the germ-plasm was a remarkable anticipation of future modes of thought. It affected the ideas underlying cytological work and the most important experimentation that has been done since the beginning of the twentieth century under the combined influence of the concept of mutation, introduced by Hugo de Vries, and the laws of Mendel which had been rescued from oblivion at the same time. On this double foundation the science of genetics has reared itself, and you are well aware of the important place that it holds in contemporaneous biology.

Let us recognize without any mental reservation or restriction whatsoever the value of the results of genetical investigations and the considerable progress that they represent. Into an indefinitely complex field like that of heredity, where empiricism reigned, gratuitous statements were made and speculative views advanced, the science of genetics has introduced methodical observation and strict experimentation. The result has been that certain general and simple laws have been detected for phenomena of heredity which once seemed essentially arbitrary. Thanks to these laws, we are now enabled to prognosticate with certainty, and this is the most unquestionable criterion of true scientific knowledge. It therefore behooves us neither to cast doubt upon nor deprecate the value of the results of genetics but only to endeavor to determine the real scope of their application.

We may say that a first series of these results, which constitutes, as it were, a gateway to genetics proper, has its source in analytical experimentation on variations and furnishes us with a more precise notion of species. Nowadays we distinguish much more clearly two categories of variations; on the one hand those that are purely individual and not hereditary, *i.e.*, of the phenotypic order, as we say. We have ascertained that in general they are due to the action of environmental factors, that is, of factors external to the organism, and, viewed statistically, obey certain simple laws and are explainable by polygons of variation or Quetelet's curves. On the other hand, there are variations due to differences in

hereditary constitution, or, as we say, genotypic. The species is not a genotypic unity but a collectivity of genotypic constitutions; the units differ from one another in details and remain distinct in cases where self-fertilization is the rule (these constitute the pure lines of Johannsen), or, in the most frequent cases where cross fertilization has taken place, they may be mixed and combine among themselves indefinitely. The Linnean species may be separated into secondary units, which are theoretically as numerous as the distinct genotypes, *i.e.,* to an indefinite number. Practically speaking, this amounts to recognizing the legitimacy of the conception of an elementary species, which had been so remarkably clarified in a series of plants by the botanist, Alexis Jordan. For this reason the name "Jordanon" has been suggested for the fundamental genotypic unity.

Obviously genotypic properties can best be studied in the pure lines. This problem in Johannsen's work has led the author to a double conclusion; first, that the genotype is altogether independent of outside influences, which merely produce the individual variations or phenotypes; and second, that selection has only the power to isolate, without changing, the genotypes which are pre-existing but confused in a population. In fact, these conclusions of Johannsen's are the result of observations on very limited material (the pedigreed culture of a limited number of strains of Princess beans during a few generations). It is certainly a very bold induction to extend—*mutatis mutandis*—to all animals and plants and even to cases of cross fertilization. Nevertheless, it may be admitted that it is likely enough that they have at least a certain general value in the present state of organisms. On the whole these conclusions amount to a deductive announcement of the fixity of the genotype; that is to say, he concludes that species are fixed in relation to their environment, and this is a downright repudiation of Lamarckism.

Genetics proper has been the experimental analysis of the genotype by the method of crosses. Its principal results are now classic. Henceforth we may consider as solidly established the fact that the constituent properties of the genotype depend primarily on the nucleus and more especially on the chromosomes. Every one knows how far Thomas Morgan and his collaborators have been able to push this analysis in *Drosophila melanogaster.* As a result of these magnificent researches, which look upon genes and their localization as tangible realities, a veritable genetical mentality has been created. These, however, have merely imaginary existence. Be this as it may of genes and their positions, the mass and precision of concordant and experimental results deduced from this conception indicate that there is at least some correlation between it and reality. Everything happens as if genes were exactly as the geneticists say they are. They permit of experimentation and prognostication, and this justifies their use, but it must not be forgotten that they are only symbols. They have permitted us to clarify the idea of mutation and its treatment. If we put aside the very special case of *Oenothera,* which has made its fortune, mutation constitutes a phenomenon, examples of which have been cited over a period of years. Darwin knew them under the name of "sport" and "single variations." We have been able to straighten out long series of facts from investigations of the nineteenth and even the eighteenth century, and some have been found of a much earlier date. A great number of our cultivated plants and races of domestic animals are mutations perpetuated from olden times by human beings. However, it is genetics alone that has yielded a good understanding of their properties, and by means of the Mendelian theory of crosses, has permitted us to tie up their breeding with the alteration of a gene or a limited but definite number of genes. They are, on the whole, genotypical variations.

The special value of mutations lies rather in their hereditary character than in their discontinuity. For if these mutations, which correspond to large variations attracted attention first, we now know that mutations can manifest themselves as small variations with the appearance of continuity. The method of crossing enables us to bring about their transmission and to group them *ad libitum* under the limitation of their correlations. You know how successfully this has been accomplished in *Drosophila.*

Knowing that mutations are hereditary variations, which represent distinct typical forms of a species, we are led naturally to look upon them as the processes by which new and lasting forms have come into being. Admitting, on the other hand, that variations due to the environment are individual and not transmissible, and that phenotypical modifications can not be repeated by the genotype, mutations appear to be the principal if not the only form of evolutional change. The Darwinian theory can be applied to them in the sense that in all produced mutations natural selection has eliminated the unfavorable to the advantage of the favorable.

Since, owing to the proof of the existence of mutations themselves and of their hereditary peculiarities, genetical laws are positive facts of experience, the doctrine of evolution appears to rest on a solid basis of experiment.

First of all, it appears from the various results of genetics that the plasticity which Lamarck attributed to species is real only from the individual and phenotypical point of view, but, when envisaged in a suc-

cession of generations, the species is on the contrary quite stable. It is characterized by a totality of genotypical and fixed attributes, and occurs in varied environments under more or less diverse phenotypical forms. The species is therefore a reality and not an individual, nor is it always susceptible to change in a continuous or arbitrary fashion under the influence of external factors which constitute the environment. The objections made by Lamarck's contemporaries to the identity of the hypogea of Egypt together with those of our time are of undoubted value. And we may even go further, since paleontology itself proves the stability of various forms during often extremely long geological periods, and sometimes even from the remotest ages. Thus there are certain ants from the amber deposits of the Baltic, that is to say from the Oligocene, which are scarcely different from the ants of the present, and therefore the typical ant was already completely and definitely developed at that time. In general, therefore, species are stable, at least in the present epoch.

Do mutations, as far as is known, actually exhibit a process leading to the formation of new species, and do they account for evolution? Such is the opinion of geneticists, and it has been developed in a masterly manner by Monsieur Emile Guyenot, professor at the University of Geneva, in a work which recommends itself by its lucidity of exposition, accuracy of documentation, and faithfully adheres to the actual facts.[2] However, I do not believe that mutations and genetical laws are sufficient to account for evolution, as it is accounted for by morphological and paleontological data.

First of all, as we know them, and as genetics has explained their growth, it is doubtful whether mutations really constitute new developments in the descent of a species. Moreover, they appear to represent rarely occurring combinations of normal elements of the genotype, that is to say, they are forms virtually contained and preexisting within the species considered as stable. Evolution, on the contrary, ought to be conceived as brought about by the production of new types which were not preceded by similar forms. The mammalian type is not really contained in the fish nor the reptilian type.

Now mutations are attributed to modifications of already existing genes and it is significant that we have almost always been forced to consider these modifications as degradations or losses. This follows from the fact that from the Mendelian point of view a large majority of known mutations behave like recessive forms. Dominant mutations are much rarer.

[2] E. Guyenot, "La Variation et l'Evolution," 2 vols., Paris, 1930 (Encyclopedia Scientifique, G. Doni, editeur).

Recessive forms are not apt to evolve since they usually will be transmitted in a heterozygote condition in which they are masked. Crosses intentionally combined by the experimenter are essential if they are to be perpetuated and revealed in the homozygote condition. Moreover, mutations which are continually occurring in a state of nature are hardly ever met with there, or fail to maintain themselves. Practically speaking, we do not find the very numerous mutations of *Drosophila* which Morgan has isolated and propagated in his cultures. Then, too, almost all the mutations obtained are malformed or weak individuals which would quickly be eliminated by selection in a state of nature. Furthermore, the better one knows the genetics of a species like *Drosophila* the more numerous appear the non-viable and lethal mutations. The number of those that are able to maintain their place in the sun without the aid of man is very small. Is there not a profound reason for this state of affairs? Is there a reason for the numerical predominance of recessive over dominant mutations and for the fact that the majority of them have defective constitutions? Is it necessary to see an essential and elementary process of evolution in a phenomenon which appears to our eyes, in the majority of cases at least, to be subpathological?

The following is an objection of another sort. Are mutations, as at present understood, really evolutionary processes, that is to say, are they capable of giving rise to distinct species and of eventually splitting up into diverse groups?

Without doubt all the mutations exhibited by a species correspond well enough morphologically to the diversity among natural species or even among allied species in the same genus. But if we examine them from the point of view of physiology we find, notwithstanding the often very considerable variation of form and structure that they assume, that they nevertheless remain strictly within the frame of the stock species. This is shown by the general and unlimited fecundity which they exhibit in crosses with the parent species. Many of Morgan's *Drosophila* mutations (like the one with vestigial wings, for example) differ in a much more striking manner from the normal form than do the various species of wild *Drosophila* (*D. virilis*, *D. confusa*, etc.). Nevertheless the close sexual attraction to the normal form holds good and fertility is wholly preserved, whereas, on the contrary, crossing does not occur or give results among wild species which are hardly distinguishable from one another. The criterion of fecundity, in spite of the numerous and important exceptions which might be cited to the contrary, is perhaps still the surest criterion of specific difference,

and the sterility of the crosses or the absence of sexual affinity is the fact which permits natural species to exist side by side. Mutations do not, therefore, result in the formation of distinct physiological populations as would true species. Therefore, why look upon them as the essential processes by which evolution, that is, the production of independent species, has been brought about? Genetics, nevertheless, readily explains why this should be so. From the genotypical point of view a mutation differs from the normal stock only by a single or a very limited number of genes; all the others remain identical. On the contrary, two closely allied natural species differ by the ensemble of their genes, that is, by their whole genome, which almost always reveals itself materially by the number of the arrangement of the chromosomes. And this gives us sufficient insight into the infertility or the limited fertility of the cross. When, exceptionally, crossing is possible and normally fertile between distinct species, as Erw. Baur has observed among species of *Antirrhinum,* then the second generation, F_2, exhibits enormous polymorphism. From one single hybrid seed (F_1) the issue of one of these crosses, Baur was able to secure in the (F_2) generation more than 150 different types. Now this is interpreted by supposing that the two crossed species differed by a great many genes, and hence a considerable multiplicity of distinct combinations among the gametes. And certain geneticists, like Lotsy, starting from this conclusion, are seeking the source of the production of new types and of all evolution in hybridization.

However, be this as it may, mutations do not arise from a process corresponding to a fundamental genotypic change of an order of magnitude like that which separates one natural species from another. Are we justified then in seeking among the phenomena of mutations for the sole and fundamental laws of evolution? Or rather are not these laws of a much more complex character, the mutations entering into them as a partial element, but without thereby implying the exclusion which genetics is leading us to formulate?

It is significant that when we have started with mutations and genetics and tried to formulate an explanation of evolution, we have only arrived at disconcerting paradoxes. This was the case of the late lamented William Bateson, one of the most eminent geneticists, and who is still considered to be one of the masters of contemporary biology. Almost all the mutations known to him being recessive, he explained them by the loss of a gene, on the genetical theory which he called "presence-absence," and which to-day I admit has been abandoned. We should have to admit, therefore, that new forms resulted from successive losses in their genotypes, that is to say, that evolutionally superior forms of the animal and vegetable kingdom are due to a progressive simplification of the initial complexity of the most primitive and inferior forms. Thus man would seem to be a simplified *ameba.* No doubt there is a touch of humor in the idea, although it revealed an embarrassment in constructing a theory of evolution on the basis of mutations.

One other grave difficulty with the conception that evolution is fundamentally the result of mutations, to the exclusion of all Lamarckian mechanism, is the explanation of adaptation, that is, the frequently astonishing structural conformity between organisms and their conditions of existence.

Adaptation, which is one of the most difficult questions in biology, has been much discussed of course in the past and even recently. Formerly naturalists saw it everywhere and marvelled at the wisdom of Providence which had endowed every living being with all the organs best suited to its rôle in nature. Such was even Cuvier's point of view. Lamarck, on the contrary, announced that adaptations were *a posteriori* results, an effect on the organisms of environmental conditions which modeled them. To-day, it is well established that adaptation is not a universal characteristic of organisms. There are many and important details in every organism that have no adaptive significance whatsoever, but which even seem to be maladapted. Obviously the organism gets along as well as it can with the organs which it possesses and its manner of living is regulated by its organs. Certain biologists are even beginning radically to reject this morphological order and to consider all evidence brought to bear in support of it as pure illusion due to coincidence. I am unable to subscribe to this point of view. An organism such as a cetacean or a bird is manifestly the result of transformations of an original type, which are strictly correlated with an aquatic or an aerial mode of life. The reality of adaptation being admitted, its realization is as difficult as it is important. The Lamarckian solution pure and simple, according to which the need suffices to create the organ, can not be adopted. Furthermore, the most clearly adaptive characteristics of the individual are apparent in ontogeny before any functioning of the organs. The mutation theory permits us to regard adaptation only as the fortuitous result of variations non-adaptive in themselves but which are preserved by selection in the event that they are found to be favorable. This was Darwin's concept also. But can we admit that a series of chances has developed such highly coordinated wholes as the body of a cetacean or that of a bird, or can

mere chance have set up certain organs which are veritable machines, or tools such as the human intelligence constructs. To explain these facts it seems to me that it is impossible to reject the direct action of the environment on organisms or the influence of the phenotype on the genotype, that is to say, a mechanism of the kind suggested by Lamarck. Every theory of evolution should be able to explain even circumscribed adaptation, but I doubt whether it can be done by genetics.

There are then undeniable difficulties in explaining evolution in general by means of genetical data and the phenomenon of mutation. Therefore, what attitude shall we adopt?

There are geneticists who remain strictly in the territory of the established facts and experimental results and who deliberately sacrifice everything that stands in their way. If the genetical mutationist can give no account of evolution, and evolution is a mere hypothesis, then there is no alternative but to sacrifice this hypothesis. Such is the conclusion at which certain eminent geneticists such as Mr. Heribert Nilsson are arriving.

Evidently there would be no disposition to accept such conclusions on the part of the majority of zoologists and botanists. For them evolution, as a fact, obviously results from all data of comparative anatomy, embryology and paleontology. Naturally I shall not enumerate the arguments, which are classic, that support this statement. Nevertheless, we are clearly unable to avoid taking account of the positive results of experimentation. But what seems to me to be of vital importance is their true significance.

However extensive experiments of this sort may be—the most ample are Morgan's on *Drosophila*—they are always extremely limited in comparison with the totality of natural facts and we are always able to ask ourselves whether the best conceived experimentation really embraces all the natural conditions, not only of actual present conditions but also those of the past. Alfred Giard, one of the biologists of the end of the last century who had the widest knowledge of zoology and botany and the most penetrating insight into evolution, was in the habit of poking fun (and with good reason) at certain botanists, who, he said, could not recognize the crucial value of an experiment unless they had repeated it in a flower pot. Nature, he added, shows us irrefutable and clearly significant experiments which she has made and which we can never dream of reproducing, but which we can not fail to interpret. Thus, by parasitism, she has converted a cirriped into a rhizocephalan, such as *sacculina*. Now all the essential phenomena of evolution are of this kind; they are experiments which require such a long time for their performance as to be impossible for us. But even if we reject this subterfuge, nature herself probably no longer performs such experiments at the present time and has realized them only at certain epochs without our being able to discover the reason. She does not keep repeating them continually. The period during which terrestrial mammals related to the carnivores passed over to a marine life and became pinnipedia and cetaceans is confined to a limited portion of the earth's history. We may make every effort to breed dogs or even such purely aquatic animals as otters during many successive generations without being able to produce seals, porpoises or anything approaching them. Moreover, it is not in a continuous or repeated manner that the species of crustaceans, cirripeds, copepods or isopods adopted a parasitic way of life and underwent the transformations which produced the rhizocephalia, the various parasitic copepoda and the epicaridae. Paleontology is a witness to the fact that all these evolutional changes are brought about in each group only during a limited period, and, in most cases, an extremely remote one. It is even conceivable that they have been evolved with relative rapidity, after which everything indicates that they had attained a perfectly stable equilibrium, as genetics has clearly demonstrated. What conditions have influenced these transformations, the reality of which is evident, we do not know. We know only that evolutionary changes are not brought about in all groups at the same time. The reptile type, which had become so highly diversified at the beginning of the Mesozoic period, remained perfectly fixed and stable at the beginning of the tertiary, in the many forms which are still surviving to-day, at the same time that the diversification and evolution of mammals was going on in large and rapid strides. Without ignoring the fact that to establish an evolution the past must be considered and the present must always, *a priori*, give an illusion of stability, we may seriously ask ourselves whether evolution has not been completely accomplished and whether the organisms whose stability and fixity are established by genetics, have not lost the mysterious ability of transformation, at least to a considerable extent, in the present stage of the earth's history. As the paleontological past teaches, the evolution of groups is limited, as Dollo has so ably contended. Evolution is not a process that is carried on indefinitely or continuously or at a uniform speed. Every group has had its stage of differentiation, after which it is congealed, as it were, into already acquired forms. We ought to ask ourselves if the characteristics we recognize in existing organisms are identical with those of the geological epochs of which paleontology gives us such

an adequate picture. It is possible that at that time the phenotype had an effect on the genotype and that, in accordance with the general sense of Lamarck's theory, if not in accordance with its formulation, an environment contributed to the modification of organisms which transformed themselves, moreover, to a great extent independently of the environment, in conformity with the correlations resulting from their intimate structure. It seems that at the present time we do not know whether stabilized nature and genetics will inform us of the modalities of this stability. Are these conclusions of genetics valid for the periods and the conditions during which each group became diversified? Or rather, as seems more probable to me, do the evolutional transformations depend on some other causes which still elude us? I am not concealing from myself the fact that it is very improper to imagine that the causes known at present are insufficient to explain the past and I ask pardon. But I still prefer to adopt such a supposition rather than to deny evolution or to confine myself to a statement of the contradictions between the results of our inadequate experimentation and the facts attested to by the past.

I ask your indulgence for having preempted your attention so long, only to end with such doubtful conclusions; but, as I said in the beginning, my intention was above all to emphasize the difficulties of the problem and to provoke reflections, suggestions and even contradictions among the experts present.

SCIENTIFIC EVENTS

THE INTERNATIONAL CONFERENCE ON BITUMINOUS COAL

THE Third International Conference on Bituminous Coal will be held at the Carnegie Institute of Technology from November 16 to 21, it is learned from Dr. Thomas S. Baker, organizer of the meeting and president of the Carnegie Institute of Technology.

Prospects for the third congress are excellent. "Because of the deep concern that is felt all over the world as a result of the great depression in the coal industry, it is felt that it is a particularly appropriate time to hold our conference," Dr. Baker said. "There has been some pressure to have the meeting postponed for another year. These suggestions have come principally from some of the European scientists, but it is thought that because of the condition of the industry it is very important that we go on with our plans."

One of the objections of the foreign scientists to coming to this country this year, apart from the difficulty of securing necessary funds, is the fact that so many industrial plants are shut down that they will be unable to study American methods of business. In spite of these conditions, there will be a larger number of European delegates than were present at the previous conferences.

"The conferences in the past have been devoted to the scientific aspects of coal utilization," Dr. Baker continued. "As this meeting is sponsored by a technological institution, the emphasis has been placed on new methods of utilizing and treating coal which are continually being developed. When the first meeting was organized in 1926, it was undertaken with the hope and expectation that it would be of service to the coal industry and the subsequent meetings have been planned with this in mind.

"Although in comparison with the previous meetings, the scientific program next November will be of equal, perhaps greater, importance, it is impossible to discuss coal at the present time without reference to the economic aspects of the industry. Therefore the various processes that will be presented will deal very definitely with economics and less with theoretical questions. There will be a certain number of papers that will be solely economic in character."

The congress will unite scientific men from all over the world, who will bring to Pittsburgh the latest developments in soft coal utilization. Some of the foreign delegates will speak also on the coal industry as a business in their respective countries, and it is felt that suggestions will be made by them that will be helpful to American coal men.

The conference will be attended by representatives from Austria, Belgium, Canada, Czechoslovakia, England, France, Germany, Italy, The Netherlands, Poland, Roumania, Spain, Sweden, Switzerland, South Africa and U. S. S. R.

PAINTINGS OF PREHISTORIC LIFE AT THE FIELD MUSEUM OF NATURAL HISTORY

THE series of twenty-eight large mural paintings depicting life on the earth in successive prehistoric ages from about one and one half billion years ago down to the beginning of the modern era, which has been in the course of preparation for the Field Museum of Natural History during the past several years, has just been completed with the installation of the last three paintings, it has been recently announced by the director of the museum.

The paintings are a gift to the museum from Ernest R. Graham, an architect of Chicago, who provided a fund of $125,000 for them and certain other material illustrating historical geology. The hall in which they are exhibited has been named in Mr. Graham's honor by the museum's board of trustees.

Charles R. Knight, of New York, known as a fore-

most painter in the very specialized field of paleontological restorations, is responsible for all the paintings. Museum authorities state that in them Mr. Knight has performed some of his most notable work.

The paintings are in two sizes, the largest being 25 by 9 feet, and the others 11 by 9 feet, being thus designed to form a continuous series about the walls of Ernest R. Graham Hall. Twenty-five previously completed were placed on exhibition and announced at various times during the past five years as they came from the artist's studio.

Of the three final paintings, one depicts the primitive hoofed animals known as Uintatheres and the four-toed horse called Orohippus which lived approximately 55,000,000 years ago; another shows flying reptiles, primitive birds and small dinosaurs of 175,000,000 years ago; and the third illustrates primitive African reptiles of the Permian age, some 215,000,000 years back.

Other subjects illustrated in the complete series include the following: The cooling earth before life began; the beginnings of the lowest orders of life; a sea beach of Ordovician time; a coral reef which existed in Silurian time on the present site of Chicago; North American reptiles of Permian time; a forest of Devonian time; large flying and marine reptiles of Jurassic time; swimming reptiles; armored dinosaurs; plant-eating dinosaurs; horned and carnivorous dinosaurs; egg-laying dinosaurs; duck-billed and crested dinosaurs; titanotheres; primitive whales; early camels and suillinos; early elephants and rhinoceroses; giant kangaroos and wombats; New Zealand moas; South American ground sloths and armadillos; saber-tooth tigers and vultures; mastodons; cave bears; mammoths and woolly rhinoceroses, and the great Irish deer.

The restorations on canvas are intended to show how prehistoric creatures are believed to have appeared when living, as indicated by careful scientific studies of fossils. In the work the artist has had the advice of Dr. Oliver C. Farrington, curator of geology at the museum, and also of many other scientists in other institutions. The series as a whole represents one of the most extensive and elaborate attempts ever made to reconstruct the prehistoric world, and is expected to be of great educational value.

COUNTY HEALTH DEPARTMENTS

"APPROXIMATELY 600 county health departments should be in operation throughout the United States before the close of 1931," Acting Assistant Surgeon Fred T. Foard, of the Public Health Service, states in a recent issue of "Public Health Reports" as reported in the *United States Daily*.

The movement for full-time county health departments throughout the country has made great progress during the nineteen years since the first full-time unit was established on July 1, 1911, in the state of Washington.

About 24 per cent. of the rural population is now being served by a health service that is reasonably effective, but in which there is still room for improvement. There are about 3,000 counties in the United States in which full-time county or district health service is applicable. The development of this tremendous field in the future can take place only as fast as personnel can be trained to take charge of the individual units.

With so great a demand for trained personnel during the next ten or twenty years, and with the many added responsibilities which are being incorporated into the public health program in increasing numbers each year, the public health official must be progressive if he would successfully meet the situation. The time when the political appointee can expect to be tolerated in the public health field without progressing with the movement is about past. The people the country over are very rapidly coming to know what the prevention of disease and the promotion of the public health mean in a literal sense.

They realize its importance both from the standpoint of the prevention of unnecessary suffering and death and from the standpoint of dollars and cents saved. Public sentiment, therefore, is demanding higher standards and more efficient health-protective service than could be given a decade ago, when public health appointments were made primarily to fulfil political obligations and, perhaps, secondarily, to the lowest bidder for the position.

Since the full-time county health department movement started a little less than twenty years ago, the national death rate from all causes has dropped from a little more than 14 per 1,000 population to 11; the tuberculosis (respiratory) death rate has dropped from 138 per 100,000 population to 68; the infant-mortality rate has been reduced from 129 per 1,000 children born to 68; the typhoid fever rate has been reduced 80 per cent., and the diphtheria rate has been reduced about 65 per cent. in the same period of time.

With such an enviable record to look back upon the public health field has greater progress to look forward to and to work for in the future. Although many of the public health executives are still handicapped by lack of funds to carry on rapidly expanding programs, it is nevertheless true that the health officer who possesses the qualifications of leadership, statesmanship and organization ability can frequently overcome handicaps which would otherwise completely retard his progress. The Public Health Service should carry on with ever-broadening viewpoints of the rapidly growing and fascinating field of public health.

administration. The old adage "There is more in the man than there is in the land" is just as true of the field of public health as it is in farming, or in any other line of endeavor.

THE BEIT MEMORIAL FELLOWSHIPS FOR MEDICAL RESEARCH

According to the London *Times*, a meeting of the trustees of the Beit Memorial Fellowships for Medical Research was held on July 14, for the election of fellows and the presentation of the annual report of the honorary secretary, Professor T. R. Elliott.

In the report of last year a review was given of the work of the Beit Memorial Trust in the first twenty years since its foundation. The close of that year marked the period that had been chosen for the review with a sharper line, by the death on December 8 of the founder of the trust, Sir Otto Beit. The founder's generous resolve to perpetuate the memory of his brother, Mr. Alfred Beit, by augmenting more than fourfold the gift which the latter had wished to make for the progress of medical studies in the University of London did not conclude with the endowment by £210,000 of the trust. As chairman of the trustees, Sir Otto devoted to the very end of his life the closest thought to every step that might advance the aims of the foundation, and he showed the keenest interest and pleasure in marking those successes in the advance of medicine by research which have been achieved by the most distinguished of the Beit Fellows. His sense of service to the trust that he had created gave an example that will not be forgotten.

The resignation of Sir Charles Martin and Sir James Kingston Fowler was announced and the appointment of Sir John Rose Bradford, who in 1909 was chiefly responsible for the advice that led to the trust being guided to the creation of fellowships rather than to other purposes, and who served for twelve years as an original member of the advisory board. Sir Alfred Beit has consented to serve with them in the place of his father, Sir Otto Beit. Dr. J. C. G. Ledingham, F.R.S., now director of the Lister Institute, who will serve in the place of Sir Charles Martin, and Dr. P. P. Laidlaw, F.R.S., known for his work on vaccination for dog distemper, have also been elected members of the advisory board.

The number of fellows at present working on the foundation, excluding those reported for election, is twenty-three. The report states that the list of places recognized for research has long ago been widened beyond the limitation suggested by the first idea that work would be done chiefly in the laboratories and schools attached to the University of London, and each year more and more fellows are being permitted for special purposes to work for a year abroad. The fellows elected choose their own problems for research, and no attempt is made by the board to select a subject or appoint a group to investigate it.

It is pointed out in the report that the aim of the fellowships is to start a man on the career of research, and the fruits of his work can hardly be expected to mature during that relatively brief tenure. It is to the after-careers that the trustees look for proof that the advisory board has guided them well in the selection of fellows. Last year's report gave a comprehensive review of the careers of the fellows since the foundation of the trust. During the present year Dr. C. H. Lambie, who held a fellowship from 1923 to 1926, has been appointed to a new whole-time chair of medicine at the University of Sydney, New South Wales. Dr. D. Keilin, F.R.S., has been chosen for the directorship of the Institute for Parasitology at Cambridge, to which he was first appointed to work as junior Beit fellow in 1920, and where he remained throughout the fourth year and senior fellowships for a total period of seven years' research. He now controls the laboratory at which his work in England began.

Besides six junior fellowships valued at £400 a year, one senior fellowship valued at £700 a year was awarded to Dr. F. R. Winton, to continue his work in the pharmacological laboratory of University College, London, on the tonus of plain muscle, on the blood pressure in the glomerulus of the kidney, and on the effect of drugs on kidney secretion. A fourth year fellowship valued at £500 a year was given to B. H. C. Matthews, to continue his work in the physiological laboratory, University of Cambridge, on the analysis of sensory nerve impulses by electrical records obtained with the delicate oscillograph which he has invented.

SCIENTIFIC NOTES AND NEWS

Dr. Harvey Cushing, head of the department of neurological surgery at Harvard Medical School, has received an honorary degree from the University of Berne at the opening of the International Congress on Neurology which met recently in Berne, Switzerland. Dr. Cushing read a paper on his experiences in two thousand brain operations.

Dr. A. S. Hitchcock, custodian of grasses at the U. S. National Herbarium, has been elected a corresponding member of the German Botanical Society.

Dr. Joseph B. Wolffe, associate professor of cardiology in the school of medicine and the hospital of Temple University, was honored on September 1 by members of the staff of his heart clinic, who enter-

tained him at a testimonial dinner in the Hotel Sylvania. Dr. Wolffe, who has passed many years in research into the causes and treatment of heart disease, returned recently from a tour of this country and South America, having visited leading heart clinics.

The Frank M. Meyer Medal for distinguished services in the introduction of new plants to America has been awarded to Allison V. Armour, of New York. The presentation was made on August 24 by Dr. Gilbert Grosvenor, president of the National Geographic Society, on board Mr. Armour's yacht *Utowana*, which was anchored off Beinn Brheagh, Nova Scotia, the summer residence of Dr. and Mrs. Grosvenor. The medal was awarded for the series of eight expeditions which Mr. Armour has made on his yacht to bring from foreign countries useful and ornamental plants to enrich the farms and gardens of the United States.

The Royal Geographical Society of Antwerp has awarded its Gold Medal to Mr. Bertram Thomas, in recognition of his explorations in Arabia.

The Planck Medal has been awarded by the Physical Society of Berlin to Dr. Arnold Sommerfeld, of the University of Munich.

The Academy of Sciences at Vienna has awarded the Fritz Pregl Prize for microchemistry to Dr. Friedrich Feigl, and the Wegscheider Prize to Dr. Philipp Gross. Both recipients are docents in the University of Vienna.

Dr. Bernard Sachs, of New York, has been elected president of the International Congress on Neurology.

At the annual meeting of the Mt. Desert Island Biological Laboratory, the following officers were elected: *President,* Clarence C. Little; *vice-president,* Duncan Starr Johnson; *treasurer,* David O. Rodick; *secretary* and director of the Weir Mitchell Station, W. H. Cole; director of the Dorr Station, R. L. Taylor, and honorary director, Herbert V. Neal. The following members were added to the board of trustees: W. H. Cole, R. W. Hegner, E. K. Marshall, Jr., S. O. Mast and H. W. Smith. New members of the corporation are: A. L. Grafflin, P. L. Johnson and D. O. Rodick.

Glenn L. Martin, aircraft manufacturer, of Baltimore, has been selected to open the sixty-seventh session of the Royal Aeronautical Society on September 16, as the Wilbur Wright Memorial lecturer, it was announced on August 25. Mr. Martin will address the society on "The Development of Aircraft Manufacturing" in the aeronautical section of the South Kensington Museum, with the permission of the director, Colonel Sir Henry Lyons, and a discussion will follow the address. At the meeting of the Royal Aeronautical Society Mr. Martin will present the Daniel Guggenheim Medal to Dr. Frederick W. Lanchester, British aviation engineer and aerodynamic expert, in recognition of his service towards the advancement of aeronautics in developing his theory of flight based on the vortex theory.

Dr. Frederick G. Cottrell, formerly director of the U. S. Fixed Nitrogen Research Laboratory, recently delivered a lecture at the Scripps Institution of Oceanography on "Research Work of the Fixed Nitrogen Research Laboratory of the U. S. Department of Agriculture."

Dr. Leslie T. Webster, of the Rockefeller Institute for Medical Research, New York, delivered a series of three lectures on epidemiology before the Pacific Northwest Medical Association at its recent Seattle meeting.

Dr. Allen F. Stone has been appointed head of the department of surgery at the University of Maryland and has also become surgeon-in-chief of the James Lawrence Kernan Hospital for the Crippled.

Dr. Joseph S. Illick, formerly state forester of Pennsylvania, has been appointed head of the department of forest management of the New York State College of Forestry at Syracuse University.

Appointments to the faculty of the Duke University School of Medicine and Duke Hospital include Dr. Bayard Carter, professor of obstetrics and gynecology, and Dr. Edwin C. Hamblen, associate professor of obstetrics and gynecology.

Dr. Virginius Elholm Brown has been appointed professor of biology and chairman of the department at Bethel College.

Dr. Helen Miller, of Johns Hopkins University, has had her National Research fellowship in zoology renewed and will spend the coming year in the department of zoology at Yale University. Dr. Daniel Raffel, a National Research fellow in the department of genetics of the Johns Hopkins University, has been reappointed and will continue his work also at Yale University.

Dr. Alvin R. Lamb, of the Wisconsin Agricultural Experiment Station, has accepted a position as biochemist in the U. S. Public Health Service. He will join the staff of the Leprosy Investigation Station at Honolulu on September 1 to work on biochemical aspects of the pathogenesis of leprosy.

Mr. Dudley Moulton has resigned as horticultural commissioner for the City of Los Angeles to accept the appointment as state director of agriculture in California.

Dr. Catherine C. Steele will become lecturer in agricultural chemistry and physics at the Horticultural College, Swanley, Kent, England.

DR. LAIGNEL-LAVASTINE, senior physician to the Hôpital de la Pitié, and secretary of the International Society of the History of Medicine, has been nominated professor of the history of medicine in the Paris faculty in succession to Professor Menetrier.

LORD ILCHESTER and Professor J. Stanley Gardiner have been elected trustees of the British Museum. They succeed Lord Ullswater and Lord Chalmers, respectively.

DR. WILLIAM R. AMBERSON, professor of physiology at the University of Pennsylvania, sailed for Germany last week. He will work in the laboratory of Professor Rudolf Hoeber in Kiel during the coming semester.

DR. W. R. JILLSON, director and state geologist of the Kentucky Geological Survey, was appointed on August 28, by Governor Flem D. Sampson, delegate to represent the Commonwealth of Kentucky at the International Geographical Congress which meets in Paris from September 16 to 24. Dr. Jillson, accompanied by Mrs. Jillson, sailed early in September. He will also represent the Kentucky Geological Survey, Syracuse University, the University of Washington and the Kentucky Academy of Science. Dr. Jillson will read a paper before the congress on "The Geography of Kentucky."

MR. RAYMOND C. SHANNON, who worked on Diptera in the U. S. National Museum some years ago and has since been continuing with mosquito work in Brazil under the International Health Board, returned to Washington on August 3 and will be in the United States for about six months. Mr. Shannon will spend most of his time in the study of mosquitoes.

DR. ROSCOE W. TEAHAN, medical director of Jeanes Hospital for the treatment of cancer in Philadelphia, has returned from a six weeks' tour for the study of cancer in medical centers of Europe.

UNDER the will of the late Payne Whitney, who left an estate valued at $200,000,000, the following institutions received legacies: The New York Hospital, $21,691,593; New York Public Library, $7,230,531; Cornell University, $3,286,605; Nassau Hospital, $657,321; Groton School, $1,314,642, and to trustees for charitable and educational purposes, $30,236,766.

THE Barker Foundation of Chicago has made a grant for research by Robert H. Gault, of Northwestern University, during a period of five years beginning on September 1. The work relates to the sense of touch and the possibility of extending its usefulness as a medium for learning to interpret and to use spoken language. A grant for the same research through a two-year period, beginning on September 1, has been made by the Illinois State Department of Public Welfare.

UNDER the will of Mrs. Mary F. W. Dickinson, $5,000 has been bequeathed to the New York Association for the Blind and $2,000 to the Chicago Nursery and Half Orphan Asylum.

THE American Hospital Association will hold its convention from September 28 to October 2. Dr. Winford H. Smith, director of the Johns Hopkins Hospital, will read a paper at the meetings.

THE meeting of the American Public Health Association opened on August 28 in Montreal, under the presidency of Dr. Hugh S. Cumming, surgeon-general of the U. S. Public Health Service.

THE German Society of Heredity will hold its meeting in Munich, from September 13 to 17. There will be a reception on September 13. On September 14, a review will be delivered by Dr. Paula Hertwig on "Artificial Production of Mutations and its Theoretical and Practical Results," followed by reading of papers. A review by Dr. Schwemmle-Erlangen on "The Relation between Cytology and Genetics in Oenothera Studies" will be given on September 15, followed by papers. On September 16, there will be three reviews as follows: von Verschuer on "Biological Basis of Human Multiple Births"; Siemens on "General Results of Human Twin Investigation"; Luxenburger on "Relation of Human Twin Investigation to Medicine." The reviews will be followed by papers.

THE nineteenth annual meeting of the Indian Science Congress, according to *Nature,* will be held in Bangalore from January 2 to 8, 1932, under the presidency of Rai Bahadur Lala Shiv Ram Kashyap. The following persons have been elected sectional presidents: Agriculture, Mr. G. N. Rangaswamy Ayyangar; mathematics and physics, Professor Ganesh Prasad; chemistry, Professor P. R. Ray; zoology, Professor D. R. Bhattacharyya; botany, Dr. Haraprasad Chaudhuri; geology, Mr. Percy Evans; medical and veterinary research, Lieutenant-Colonel A. D. Stewart; anthropology, Mr. J. P. Mills; psychology, Professor N. S. N. Sastry.

THE first Congress of Medical Geography will be held at Geneva in October. The subject for discussion will be hepatic cirrhosis.

THIRTY-SEVEN countries were represented at the Berne Hygiene Exposition and International Congress on Neurology, which met at the University of Berne, from August 31 to September 5. Professor W. Sachs, of New York, presided over the 1,200 delegates from America and Europe. The president of the Swiss confederation was honorary chairman.

THE second International Congress of Tropical Medicine will be held in Amsterdam, Holland, from

September 12 to 17, 1932, under the presidency of Professor G. Grijns. The main subjects to be discussed at the general session of the congress are: (1) Avitaminoses with special reference to beri-beri, (2) yellow fever and 2b leptospiroses, (3) helminths with special reference to hookworms, and (4) malaria with special reference to blackwater fever. Special speakers have been invited for each of the above subjects. In addition to the general meeting, section meetings will be held at which there will be an opportunity to discuss other subjects. Persons desiring to participate in the congress should notify the general secretary, Professor E. P. Snijders, Institute of Tropical Hygiene, Amsterdam, Holland, not later than December 31, 1931. The subscription fee is one pound sterling (12 Dutch guilders), payable at the Nederlandsche Bank, Amsterdam.

According to *Industrial and Engineering Chemistry*, the van't Hoff Fund, founded to endow investigators in the field of pure and applied chemistry, has available for 1932 approximately 1200 Dutch guilders. Applications must be sent by registered mail to Het Bestuur der Koninklijke Akademie van Wetenschappen, bestemd voor de Commissie van het "van't Hoff-Fonds," Trippenhuis, Kloveniersburgwal, Amsterdam, Holland, and must be received before November 1, 1931. Applicants should give a detailed account of the proposed use of the grant and of the reasons upon which the claim is based. Copies of papers resulting from the work must be sent to the committee, but may be published in any journal, with a note to the effect that the work was supported by a grant from the van't Hoff Fund.

Before attending the meeting of the American Public Health Association in Montreal, Canada, an official delegation of physicians, representing the Association of Medical Officers of Health of Great Britain, is making a study of federal, state and municipal health organization and administration in the United States.

The trustees of the University of the Philippines have closed the college of dentistry "because disputes had destroyed the usefulness and efficiency of the college."

Ritter Hall, the new $120,000 laboratory at the Scripps Institution of Oceanography, opened about September 1. The new building will make available an additional 14,000 square feet of laboratory space, which will augment the laboratory space in the library building and in the George H. Scripps laboratory building. The new laboratory occupies a ground space of 46 by 100 feet and is three stories in height. The first floor contains laboratories equipped with tanks for salt-water fish, a laboratory for photographic purposes, a twelve-unit refrigeration department, in which salt brine is maintained at an even temperature, a transformer vault, boiler rooms, a carpenter shop and workrooms.

A series of illustrated lectures will be delivered in the lecture hall of the Museum Building of the New York Botanical Garden at 3:30 as follows: September 5, "Microscopic Projection of Plant Sections," Professor W. J. Bonisteel, professor of botany and pharmacognosy at Fordham University; September 12, "Flowers in Late Summer Gardens," Mr. Kenneth R. Boynton, head gardener; September 19, "The Gardens of the World," Colonel E. A. Havers; September 26, "Color Photography in the Garden," Mrs. Jerome W. Coombs, of Scarsdale; October 3, "Dahlias," Dr. Marshall A. Howe, assistant director; October 10, "Plant Distribution in Malaya," Dr. Elmer D. Merrill, director-in-chief; October 17, "Autumn Coloration," Dr. A. B. Stout, director of laboratories; October 24, "Botanical Activities in the United States," Dr. John Hendley Barnhart, bibliographer; October 31, "The Ancestral History of Some Living Plants," Dr. Arthur Hollick, paleobotanist; November 7, "Autumn in the Garden," Mrs. Wheeler H. Peckham, honorary curator, iris and narcissus collections; November 14, "Some Edible and Poisonous Mushrooms from Maine," Professor H. Beaman Douglass; November 21, "A Botanist's Rambles through the West Indies," Mr. Robert Hagelstein, honorary curator; November 28, "A Winter in Bermuda," Dr. Fred J. Seaver, curator.

DISCUSSION

SUBMERGED PEAT BEDS AMONG THE APOSTLE ISLANDS

In Science for February 13, 1931, Dr. L. R. Wilson, of the University of Wisconsin, discusses evidences which seem to show a recent lower lake level in the western part of the Lake Superior basin. The evidences consist of peat and overlying sand that was dredged from the bottom of the lake at two localities among the Apostle Islands. It is stated that the peat was brought up from a depth of 54 feet and was covered by about 14 feet of lake sand. It is stated, further, that upon analysis the peat was found to be very fresh and that the sand shows characteristics which indicate that it was deposited since the retreat of the last ice-sheet. Dr. Wilson ventures the opinion that the occurrence of this peat in the situation described "is not in accordance with our present ideas concerning the post-glacial history of the region." I would like to point out that, instead of being out of accord with our present ideas, the submerged peat

beds among the Apostle Islands appear to constitute by far the best evidence yet found in support of our present interpretation of the lake history.

Dr. Wilson gives a reference to Dr. Frank Leverett's recent report on the "Moraines and Shore Lines of the Lake Superior Region" (U. S. Geological Survey, Professional Paper 154-A, 1929). In this paper, Dr. Leverett makes the statement that after the outlet of the Nipissing Great Lakes had become established at North Bay, Ontario, ". . . the differential uplift raised the outlet at North Bay so high that the lake waters were brought up to the St. Clair outlet at Port Huron. By this rise any shore work done by the Nipissing Great Lakes south of the isobase that runs through the North Bay outlet would have been submerged and to a large degree obliterated. The original Nipissing beach is to be seen, if anywhere, only in the extreme northeastern part of the Lake Superior basin. The visible Nipissing beach is therefore, in the main, the product of the shore work after this rise, at a time when both the North Bay and the St. Clair outlets were in use" (page 70).

Thus, the *original* Nipissing beach marks the first stage of the Nipissing Great Lakes, and the *visible* beach (commonly called simply "the Nipissing beach") marks the second stage. The first stage had one outlet, and that was at North Bay, Ontario; the second had two outlets, for the gradual elevation of the land in the north backed the water up toward the south and finally caused an overflow into the St. Clair River at Port Huron, Michigan. Continued uplift finally diverted the whole discharge from North Bay to Port Huron, ending the Nipissing Great Lakes and inaugurating the present or post-Nipissing lakes.

The Nipissing beach at North Bay has an altitude of 698 feet above sea-level. Studies along the north shore of Lake Superior by A. C. Lawson in 1892 and by the writer in 1895 furnished data for drawing the isobase of the North Bay outlet. It runs west-northwest (about N. 68° W) from North Bay and passes close to Mazokama on the north side of Nipigon Bay. This isobase cuts off about 50 miles of the present shore of Lake Superior in its extreme northeastern part, and it is only in this stretch north of the isobase that the first or original Nipissing beach can now be seen. Everywhere south of the isobase, the original Nipissing beach is submerged, and the visible shore line in that area is in reality the beach of the later, two-outlet stage. Thus, southward from the isobase the plane of the original Nipissing beach passes more and more deeply under the plane of the later or two-outlet stage. This relation of the two lake stages and of the beaches which represent them was well established in 1895, but the amount of submergence at any given place south of the isobase has remained unknown for lack of clearly defined evidence. The submerged peat bed of the Apostle Islands appears to supply the first reliable evidence bearing on the depth of submergence of the original Nipissing beach.[1]

Knowing the characteristics of peat and the normal conditions of its growth, we seem justified in believing, tentatively, that the peat described by Dr. Wilson was formed at or very near to the level of the Nipissing Great Lakes during their first or one-outlet stage. Peat is formed mainly in clear, quiet waters free from notable disturbance and from all sedimentation, whether by wave action and shore currents or by streams. It is, of course, conceivable that the peat in question was formed in a bog, in a drainless basin on a post-glacial land-surface, but it seems much more probable that it was formed in a lagoon close to the lake shore of that time, protected from wave-and-current sedimentation by a bar between it and the lake, and at the same time receiving no stream-borne sediment from the adjacent land. From these considerations it seems probable that the submerged peat west of Sand Island lay very close to the lake level of that time, probably within a foot of it or less.

Dr. Leverett found that the plane of the two-outlet stage intersects the present lake surface about on a line running west-northwest from Washburn, Wisconsin, and meeting the north shore near Knife River, 20 miles northeast of Duluth. From this line the imaginary plane of the two-outlet stage rises toward the north-northeast about six inches per mile to the isobase of North Bay. Dr. Wilson states that the peat locality is about one and one half miles west of Sand Island, which would place it about 15 miles northeast of the isobase of Washburn. The plane of the two-outlet stage passes, therefore, over the peat locality about seven or eight feet above the present lake surface, or something like 62 or 63 feet above the peat, thus affording a tentative measure of the interval between the plane of the original Nipissing beach and the plane of the two-outlet beach at that particular place.

If a line parallel to the isobase of North Bay be drawn through the peat locality and produced toward the southeast it passes over the northern part of Lake Michigan and the southern part of Lake Huron. These lakes are both 20 feet lower than Lake Superior, so that, if we assume, tentatively, that the submerged plane of the original Nipissing beach passes uniformly through all three of the lake basins, this beach should be found at a depth of about 34 feet in the basins of Lakes Michigan and Huron on the

[1] The history of the Nipissing Great Lakes and the relations of their beaches and outlets are discussed in U. S. Geological Survey, Monograph 53, Chapt. XXII, especially pages 456–7, 1915.

isobase of Dr. Wilson's peat locality in the Apostle Islands.

In the Lake Huron basin the isobase of the peat locality produced from Lake Superior passes roughly 40 miles north of the zero or hinge-line of the northern uplift. At and south of this line there appears to have been little or no uplifting of the land since the beginning of the Nipissing Great Lakes, so that in this interval, or more probably in the southern part of it, the uplift died out, and the plane of the original Nipissing beach meets the subaqueous slope or, if the depth of the basin permits, becomes horizontal at the hinge line and continues southward in that attitude. The depth of the submerged horizontal stretch is not now known, but seems likely to be near 40 or 45 feet.

In the Professional Paper referred to above (pages 71-2), Dr. Leverett refers to G. R. Stuntz's finding of submerged tree stumps in place at the mouth of St. Louis River west of Duluth. In the area southwest of the Washburn isobase, the plane of the beach of the two-outlet stage passes below present lake level. The drowning is probably 12 to 15 feet at the river's mouth. Since the slow uplift turned the narrow strait at Sault Ste. Marie into a river, the level of Lake Superior has been controlled by that barrier. Its isobase, parallel with that of the North Bay outlet, strikes the north shore near Grand Portage Bay, Minnesota. In the whole area south of this line the shores of the lake are now undergoing progressive drowning. If the submerged peat west of Sand Island lies close to the level of the original Nipissing beach this fact affords a more accurate basis for the study of many interesting problems relating to the history of the Nipissing Great Lakes. The occurrence of the submerged peat in the Apostle Islands is decidedly in accord with our present knowledge of the post-glacial history of the region.

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NORTH AMERICAN PHYLLOPODS

In Science, February 27, 1931, Mr. C. H. Behre, Jr., has presented some interesting questions concerning the zoogeography, ecology, and natural history of *Branchinecta*. His paper has prompted the following notes and discussion.

The occurrence of *Branchinecta coloradensis* at high altitudes is not necessarily an index of relationship. If the species in question had resulted from isolation since the glacial epoch, it should be related morphologically to the species *B. paludosa* of the arctic. The genus *Branchinecta* has representative members in Brazil, Patagonia, Russia, Hungary, Australia, Asia Minor, Mongolia, Tibet, United States and the circumpolar regions of the north and south. In all, about thirteen species are now known, five of which occur in North America. The relationships of the various species have not been adequately considered. However, there is no more reason to believe *B. coloradensis* morphologically related to *B. paludosa* of the arctic than *B. granulosa* from Patagonia, *B. packardi* from Colorado, or *B. ferox* of Russia, Hungary and Asia Minor.

In a recent paper[1] I have shown the distribution of the two continental species of *Streptocephalus*. Since that time I have collected *S. sealii* in small pools in the jungle between Jalapa and Vera Cruz in the state of Vera Cruz, Mexico. *S. sealii* is now known northward as far as Medicine Hat, Alberta and southward to the *tierra caliente* of Vera Cruz, Mexico. The ranges of the two continental species overlap on the central plateau, but the remarkable thing ecologically is, that, as yet, the two species have not been taken together in the same pool.

Thamnocephalus platyurus, Streptocephalus texanus, and *Apus aequalis,* were taken near Cerritos, San Luis Potosi, Mexico, in a roadside pond on June 2, 1930. *Leptestheria compleximanus* was obtained on May 14, 1930, in pools of the Lago de Texoco in the Federal District, Mexico. A species of *Apus* found in the same region is, according to the peons, used as food during the occurrence of the phyllopod in the winter months.

Concerning the question raised by Mr. Behre of the method of establishment of a phyllopod fauna, it should be noted that probably all phyllopod eggs can withstand desiccation. Many possibilities exist for transportation of eggs. Wading birds, turtles, mammals including man, are possible transporters. We do not know, as yet, whether or not the eggs of phyllopods are viable after passing through the intestinal tract of vertebrate animals Many species of entomostraca have been reared from mud obtained by travelers in foreign lands. G. O. Sars has written several papers on entomostraca obtained in this manner.[2] This suggests the means by which ponds may become stocked with fairy shrimp and other entomostraca.

My observations with *Eubranchipus vernalis* lead me to believe that death normally occurs shortly after breeding. The males die first, and this is the probable explanation of the statement so often found in the literature on various species of Phyllopoda: the male of this species is unknown. A resting period of unknown length is necessary before the eggs can hatch. Thereafter the appearance of the fairy shrimps is

[1] E. P. Creaser, Occ. Pap. Mus. Zool. Univ. Mich., No. 217, 1930.

[2] G. O. Sars, *Skr. Vidensk. Christiania*, 95, 8, 1–56. *Arch. Naturv.*, 18, No. 2, 1–17. *Ibid*, No. 3, 1–81.

governed ecologically by factors such as temperature, rainfall, and the chemical constituents of the environment. In some instances drying of the eggs appears necessary.

It may be of interest to note that in the collections of the University of Michigan Museum of Zoology there are some specimens of *Branchinecta coloradensis* taken from a hollow in a boulder. These were obtained on Sept. 14, 1916, at Estes Park, Colorado, at an elevation of 8,000 feet.

It is quite apparent that the phyllopods are zoogeographically unruly. Ecologically they are profoundly interesting, and they certainly merit, as Mr. C. H. Behre, Jr., says, a detailed study of their life history.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A TECHNIQUE FOR FIXING AND REMOVING CHICK BLASTODISCS AND EMBRYOS FROM THE EGG SHELL

FOR some time I have been looking for a simple, rapid and effective method for removing blastodiscs and embryos from the egg shell which might be used both by the technician in making up quantities of whole mounts or serial sections, and one which can be readily acquired by students in the classroom who have had very little or no training in handling such delicate objects. The method outlined by Dr. M. F. Guyer in his "Animal Micrology" (p. 127) is very good, but in order to use it the worker must have had considerable experience. Because of this I have found it impracticable to use in the classroom when both time and material are limited.

The technique outlined by Dr. C. E. McClung in his "Handbook of Microscopic Technique" (p. 213) is the best I have so far been able to find, but in using the formal-nitric fixative recommended (three parts ten per cent. formalin to one part ten per cent nitric acid) I experienced difficulty in removing the vitelline membrane, and also in washing the blastodisc free from yolk. The length of time required by the McClung method for the fixative to act sufficiently to permit the removal of the blastodiscs or embryo was from fifteen to twenty minutes. During the past year and a half I have prepared several hundred whole mounts and serial sections of blastodiscs and embryos. By modifying the formal-nitric fixative suggested by Dr. McClung from three parts of 10 per cent. formalin and one part 10 per cent. nitric acid to four parts of 10 per cent. formalin and one part of 20 per cent. nitric acid, I have found that I can remove as many as sixty blastodiscs or embryos up to ninety-six hours' incubation within an hour and a half with the aid of an assistant, whereas the original method as outlined by Dr. McClung required from four to five hours to prepare the same number. Comparative studies of tissue fixed by the two different concentrations of the fixative did not reveal any differences in their effect upon cell structure.

The following is a summary of the procedure I followed:

(1) Incubate the egg to the desired stage.

(2) Remove the egg from the incubator to a finger-bowl filled with normal salt solution. (Seven to nine grams NaCl per liter of distilled water.)

(3) Using a pair of small forceps and curved scissors, remove the upper part of the egg shell to expose the blastodisc or embryo.

(4) Flood the blastodisc with several drops of the formal-nitric fixative. Remove the coagulated albumen and apply more of the fixative. Repeat this operation until the blastodisc is free from albumen. When all of the albumen is removed flood the blastodisc or embryo with the fixative and permit it to act for from one half to two minutes.

(5) Cut around blastodisc with curved scissors.

(6) By inserting a section lifter into the yolk under the blastodisc, lift the latter and transfer it to a Syracuse watch-glass filled with normal saline. Agitate the salt solution until the blastodisc is washed free from the yolk, and then transfer it to a watch-glass of fresh salt solution.

(7) Remove the vitelline membrane by grasping its free edge with forceps and move the membrane gently back and forth until it becomes free.

(8) Transfer the blastodisc, washed free from yolk and vitelline membrane, to a dish of Worchester's fluid.[1] (Nine parts ten per cent. formalin saturated with mercuric chloride to eleven parts glacial acetic acid.) It is desirable to place about a quarter of an inch of the fixative in a rather large flat-bottomed dish in order that the embryonic tissue being fixed will lay flat.

(9) Place the tissue in seventy per cent. alcohol containing iodine colored to that of port wine to remove the corrosive sublimate. (Three to ten hours.)

(10) Preserve in fresh seventy per cent. alcohol until ready for use.

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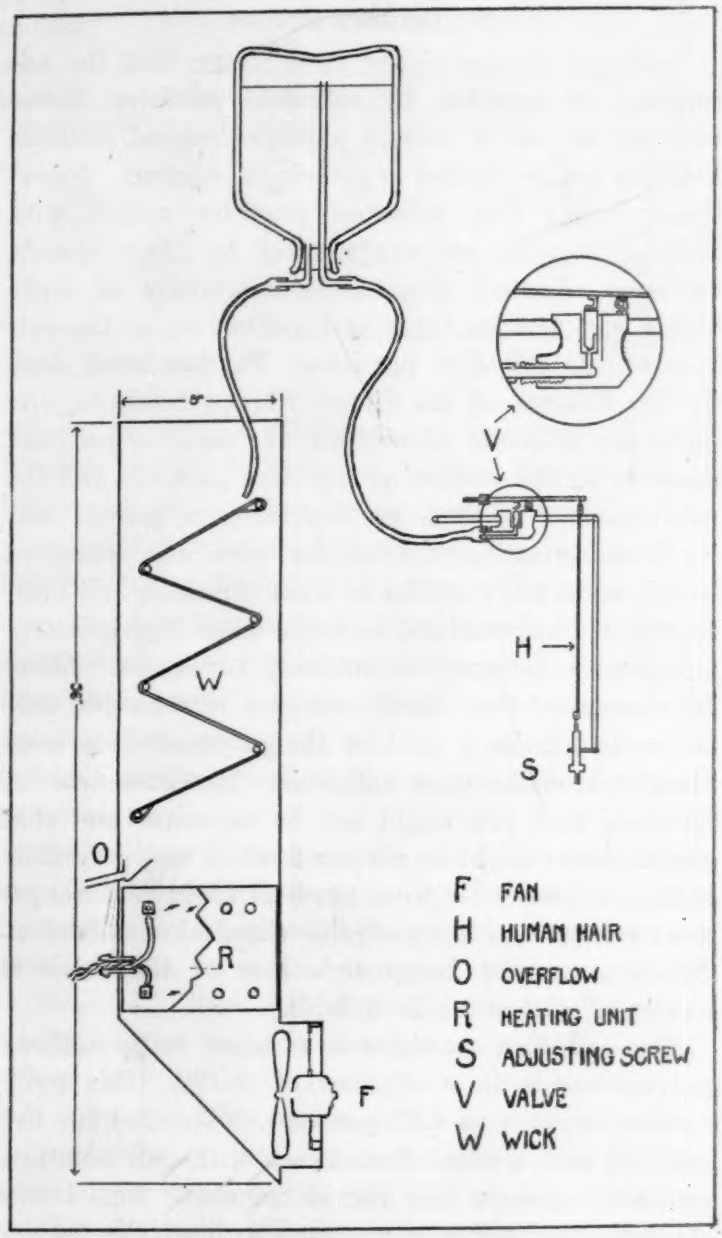
[1] Fixatives other than Worchester's may be used if desired, but for total mount work Worchester's is preferable since it has a tendency to leave the tissue tough rather than brittle as do many fixatives.

A HEATING-HUMIDIFYING DEVICE FOR INCUBATORS

THE apparatus described herein is believed to fill a long-felt want in biological laboratories for a cheap and efficient heating and humidifying unit for incubators. The original unit cost approximately $12.00, including the six-inch fan. It has been in constant use for several months in an incubator of forty cubic feet capacity used for insect rearing work and has given complete satisfaction.

The chief features of the apparatus may be summed up as follows: (1) Heating, humidifying and circulation of the air in the incubator are all taken care of by one compact unit. (2) Any reasonable temperature above that of the surrounding atmosphere may be automatically maintained as accurately as the thermoregulator used will permit. (3) Any relative humidity above that of the surrounding atmosphere up to 95 per cent. may be maintained automatically, and independent of variations outside. (4) The fan in the unit maintains sufficient circulation in the incubator to insure uniform conditions throughout. (5) The maintenance and regulation of the humidity is independent of the heating device and may be used to maintain a constant humidity while the temperature is allowed to vary.

The unit, as illustrated in the diagram, consists of a metal box about three feet long and eight inches square, and open at both ends. The bottom opening is at the side and is made round to accommodate the blades of a six-inch electric fan, F. This fan operates constantly at a reduced speed. The heating unit, R, consists of a coil of resistance wire wound to three hundred watts and surrounded on the sides by an asbestos casing. This is connected to the electric service through an electric thermoregulator, which may be placed in any suitable place in the incubator. Above this and supported on brass rods is a zig-zag porous cotton wick, W. Water drips on the top of this wick from a tube connected to an inverted bottle which rests on the top of the incubator or in any convenient place which is slightly above the unit. The water thus fed to the wick is carried off to the incubator by evaporation in the air current produced by the fan. Water can only leave the bottle as air is admitted. The admittance of air is governed by the valve, V. This is made from old carbureter parts and is opened and closed by the contraction and expansion of strands of human hair, H, the same as used to activate the hand on a standard hygrograph. Adjustment is made by the screw, S, and the percentage humidity determined by testing with a hygrograph or wet and dry bulb thermometers. O is a small overflow to take care of any surplus water which may come down, caused by the valve opening wide when the door of the incubator is kept open unduly long and the air in the incubator has become dry.



A more complete description of the apparatus with photographs will be published later along with hygrothermograph charts indicating the capabilities of the instrument. To date the tests it has been possible to give the apparatus have been somewhat limited, due to the necessity of keeping it in constant use for a particular type of work. It has, however, demonstrated its ability to maintain an even humidity of over 90 per cent. when the room humidity was as low as 40 per cent., both with and without constant heat.

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SPECIAL ARTICLES

COMBINATION OF BACTERIAL POLYSACCHARIDES AND COLLODION PARTICLES AS ANTIGENS

Preliminary Report

Loeb and his associates[1] have shown that the adsorption of proteins by collodion particles makes possible the use of certain physico-chemical methods that can not be applied to protein in solution. Jones[2] demonstrated that collodion particles coated with various proteins are agglutinated by their specific antisera. Freund[3] studied the adsorption of diphtheria and tetanus toxin and antitoxins on the surface of the collodion particles. Further work done by Dr. Freund, of the Henry Phipps Institute, and ourselves with the adsorption of bacterial polysaccharides on the surface of collodion particles and the agglutination of them by immune sera proved that its homologous polysaccharides were also adsorbed by the collodion particles as were the toxin and antitoxin. Polysaccharides, as such, when injected have not proven to result in antibody formation. Many have assumed that fairly complex nitrogenous substance had to be a part of the polysaccharide combination to make these antigenic. It occurred to us, however, that this might not be necessary and that combinations might be prepared which may owe their antigenic properties to a physical basis or arrangement rather than to an organic chemical combination. (I am aware of Langmuir's idea of the chemical nature of adsorption in colloids.)

The collodion particles are mixed with anthrax polysaccharide in a dilution of 1–100, (this polysaccharide contains 0.03 per cent. N) cooled for five minutes and washed four times with salt solution, centrifuging each time for 10 minutes. This heavy suspension of particles was standardized (by turbidity) to an approximate concentration of 5,000,000,000 per cc. Agglutination tests were made in test-tubes, diluting the serum up to 1–64 and adding an equal amount of the collodion particle suspension. Incubated for 2 hours at 37° C. The reading was checked with a hanging-drop under the microscope. Proper controls were made with non-homologous polysaccharide adsorbed to the collodion particles, as well as using normal horse serum and other heterologous sera for the agglutination test. We obtained specific agglutination in a dilution of 1–32 and sometimes in a dilution of 1–64, with the collodion particles adsorbed with anthrax polysaccharide and its specific antiserum.

For our immunization experiments, several combinations of adsorbed substances to the collodion particles were prepared:

(1) Collodion particles adsorbed with 1 per cent. anthrax polysaccharide.

(2) Collodion particles adsorbed with normal horse serum, washed 4 times, then adsorbed with anthrax polysaccharide.

(3) Collodion particles adsorbed with anti-anthrax globulin, washed, and then adsorbed with anthrax polysaccharide.

(4) The washed precipitate of anti-anthrax globulin and anthrax polysaccharide, without collodion particles.

In the first case, we wanted to find out the results of the polysaccharide alone adsorbed to the particles. In the second case, normal horse serum adsorbed first, washed 4 times, and then the polysaccharide, hoping to find out the effect of a foreign protein and a polysaccharide combination, and lastly adsorbing specific antiserum first, then washing, and followed by the specific polysaccharide with the object of increasing the amount adsorbed according to the findings of Freund in working with diphtheria toxin, where he found that collodion particles, treated first with antitoxin and then with toxin, were slightly but definitely more toxic than particles treated with toxin alone. As controls, rabbits were immunized with the suspended washed specific antibody precipitate obtained from immune serum and its specific carbohydrate. It was not thought necessary to try rabbits immunized with polysaccharide solution alone, as other investigators as well as ourselves have failed.

The rabbits were immunized by injecting intravenously 0.5 cc, 1.0 cc and 1.0 cc on consecutive days and rested for 5 days. A second and third series were made increasing the dose to 3 cc. Seven days later they were trial-bled and agglutination and precipitin tests were made. The results are given in Table I.

The variation in titer of the different series is thought to be due to variation in the response of the individual animal, although the animals injected with the antibody precipitate seemed to give higher value. For the present we are not giving special significance to this finding. The third series also seems to be better than the first.

To ascertain if the foreign protein in the second, third and control series had given anti-horse antibodies, specific horse precipitin was tested for and positive reaction found in all the rabbits injected, except those which received collodion particles and polysaccharide alone. The presence of specific anti-horse precipitins demonstrate that the material ad-

[1] J. Loeb, "Proteins and the Theory of Colloidal Behavior," 1922.

[2] F. S. Jones, *Jour. Exper. Med.*, XLVI, 303 (1927): *Ibid.*, XLVIII, 183 (1928).

[3] J. Freund, *Proceed. Soc. for Exper. Biol. & Med.*, 1930, XXVIII, 65.

TABLE I

AGGLUTINATION TESTS WITH SERUM OF RABBITS IMMUNIZED WITH COLLODION PARTICLES ADSORBED WITH ANTHRAX POLYSACCHARIDE AND OTHER COMBINATIONS

Injected with	C. particles and anthrax SSS			C. particles and antianthrax Gl. and anthrax SSS			C. particles and normal horse serum and anthrax SSS			Anthrax Gl.-anthrax SSS ppte		
Rabbit No.	6704	6705	6706	6707	6708	6709	6713	6714	6715	6710	6711	6712
Final serum dilution 1/4	4	4	3	4	4	3	4	4	4	4	4	4
1/8	4	3	2	4	4	3	2	3	4	4	4	4
1/16	4	2	2	4	4	2	1	3	2	4	2	4
1/32	4	1	1	2	4	1	0	2	1	1	0	3
1/64	2	0	0	2	2	0	0	0	0	0	0	1

Incubated over night in water bath at 37° C.

sorbed on the particles is available for stimulating antibody response.

These results may offer an explanation of why polysaccharides and other substances (of the class of haptenes) are not antigenic when injected alone in solution in the body. The collodion particles provide an enormously increased surface to the polysaccharide (haptene) and make them available in a different physical form on injection. Other factors no doubt come in.

Further work is being done on many of the problems here suggested, especially in finding other colloids which may be employed for the same purpose, as well as the immunization of different animals with various bacterial polysaccharides. Work is in progress with the protection of mice against pneumococci with the specific carbohydrate to collodion particles. At the same time we have adsorbed several polysaccharides in the same collodion particles with the hope of forming a multiple antigen. The result of all this work will be published later.

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LIGNIN AS A NUTRIENT FOR THE CULTIVATED MUSHROOM, AGARICUS CAMPESTRIS

THE cultivated mushroom, *Agaricus campestris,* in common with other fungi, must obtain all the nutrients necessary for its growth and reproduction from the organic matter of the substrata upon which it grows, this organic matter also serving as a source of energy. Horse manure is commonly used as the source of organic matter for the cultivation of the mushroom, although recently attempts have been made to replace this manure by composts of so-called "artificial manure" prepared from straw and other plant residues, from peat, etc. The chief point to be considered in the use of manure or composts of plant residues is that, in order to obtain a fair growth of the mushroom, the manure and residues must first be thoroughly composted; this process lasts, under favorable conditions, from 4 to 12 weeks. The microorganisms active in the composts bring about a number of chemical changes in the various organic complexes; these can be briefly summarized as a reduction of the water-soluble substances, of the hemicelluloses and cellulose, and an increase in the lignin, ash and protein content.

The problem is to determine which of these chemical constituents in the compost form the nutrients for the mushroom, whether all the organic complexes are attacked alike, or whether some are acted upon to the exclusion of others. This problem is both of theoretical interest and has considerable practical application.

It is known, from studies of the activities of wood-destroying fungi, that some organisms attack the cellulose of the wood, but not the lignin, while a few are capable of attacking the lignin as well as the cellulose. With the exception of these wood-destroying fungi, very few organisms are known so far capable of decomposing lignin and using this material as a source of energy and as a nutrient material. Although lignin has been shown to be generally much more resistant to decomposition by microorganisms than celluloses and hemicelluloses, as shown by its relative persistence in composts, peat bogs and soil, it still undergoes some decomposition, especially under aerobic conditions; otherwise the whole surface of the earth would soon be covered with lignin, since all plant residues, with the possible exception of the algae, contain from 5 to 30 per cent. lignin.

In order to illustrate the changes that take place in the composting of stable manure, a typical analysis of a compost of horse manure is given Table I. If one assumes that the concentration of the mineral

TABLE I

CHANGES IN THE CHEMICAL COMPOSITION OF HORSE MANURE PRODUCED BY MICROORGANISMS IN THE PROCESS OF COMPOSTING

Chemical constituent	Fresh manure	Compost 27 days old	Compost 108 days old
Total nitrogen	1.58	2.05	2.62
Ether soluble portion	1.85	1.28	0.70
Cold water soluble portion	4.45	6.64	5.11
Hot water soluble portion	2.95	7.43	3.85
Hemicelluloses	18.84	15.26	11.74
Cellulose	27.41	20.41	12.00
Lignin	17.69	19.98	22.30
Water-insoluble protein	8.25	11.50	13.56
Ash	12.51	17.80	25.50

constituents of the manure increase in inverse proportion to the reduction of the organic constituents, the doubling of the ash content of the compost within 108 days indicates a reduction of the total organic matter to one half of the original material. Of the four most important groups of chemical complexes of the manure, namely, the hemicelluloses, cellulose, lignin and protein, the first two diminished in relative concentration, especially the cellulose, while the last two increased in proportion. It is these two complexes,

TABLE II

DECOMPOSITION OF THE VARIOUS CHEMICAL CONSTITUENTS OF FRESH AND COMPOSTED MANURE BY AGARICUS CAMPESTRIS

Chemical constituent	Fresh manure: Proximate composition per cent. of dry material		Total amount in culture per cent. of original	Composted manure: Proximate composition per cent. of residual dry material		Total amount in culture per cent. of original
	Control	Culture		Control	Culture	
Total material	....	....	94.0	....	....	90.6
Total nitrogen	1.44	1.65	107.2	1.5	1.6	96.0
NH_3–N per cent. of total N	....	....	....	2.7	5.2	192.6
Ether-soluble	2.3	1.4	57.2	0.7	0.6	72.9
Hot water soluble organic matter	5.6	11.9	199.3	3.6	10.5	264.7
Water soluble nitrogen per cent. of total nitrogen	25.3	40.0	....	15.2	39.4	....
Hemicelluloses	18.6	14.4	72.8	7.1	6.6	83.2
Cellulose	20.7	20.8	94.5	14.8	16.7	101.9
Lignin	20.0	15.7	73.8	20.8	14.8	64.5

namely the lignin and the protein, which are used predominantly by the mushroom fungus for their growth and activities, as shown in Table II.

It is important to note that, as a result of the growth of *Agaricus campestris* upon fresh and composted manure (these experiments were carried out by inoculating sterilized manure with pure cultures of the organism), there was very little reduction in the total weight of the compost; the actual loss was only 6 to 9.4 per cent. of the original material. This reduction is only apparent, since the mushroom fungus synthesizes an extensive quantity of mycelium, which compensates for a large part of the organic matter of the compost which has been lost through decomposition. This is illustrated by the marked increase of the water-soluble organic matter and the nitrogen as a result of the growth of the fungus; the increase in these complexes is due entirely to the fact that over 40 per cent. of the mycelium of the fungus, on a dry basis, is water soluble.

Of the more important organic complexes, the cellulose was either not attacked at all by the mushroom, as in the case of the composted manure, or reduced only to a limited extent. The hemicelluloses were markedly reduced, especially in the fresh manure. The most striking phenomenon is observed in the reduction of the lignin, which forms the most abundant group of organic complexes in the composted manure and of which more than one third was decomposed by the mushroom in 47 days. The reduction of the lignin in the composted manure accounts, in these experiments, for nearly 80 per cent. of the total reduction in the weight of the original material. There is no doubt that the protein of the compost has also undergone considerable change, as shown by the increase in ammonia and in water-soluble nitrogen. The fact that the mushroom contains about 6.5 per cent. total nitrogen, a large part of which is soluble in water, is responsible for this.

One may, therefore, feel justified in concluding that *Agaricus campestris* feeds largely upon the lignin and protein of the manure and to a less extent upon the hemicelluloses, cellulose and other complexes. Further, that the need for the composting of manure, in order to develop a favorable medium for the growth of mushrooms, consists in bringing about an enrichment in the lignin and protein content and possibly in a change in their chemical nature.

A detailed discussion of these experiments and their bearing upon the problem of mushroom nutrition will be published later.

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